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Experiment and Theory in Particle Physics: Reflections on the Discovery of the Tau Lepton*

Martin L. Perl
Stanford Linear Accelerator Center
Stanford University
Stanford, CA 94309

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MARTIN L. PERL
Stanford Linear Accelerator Center
Stanford University
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Contents

1 The State of Elementary Particle Physics

This paper is not explicitly about the discovery of the tau lepton, I have related that history in my Nobel lecture [1] and in my book Reflections on Experimental Science [2]. This paper contains some of my thoughts on doing experimental science; thoughts that came from reflecting over the past few years on the tau discovery and on other of my experiments - good ones and bad ones [2]. How do we get good ideas in science, ideas that lead to significant progress? Are there ways to stimulate the creation of good ideas and the accomplishment of significant progress in our field of elementary particle physics?

This meeting of the Division of Particles and Fields of The American Physical Society is wonderfully active, the sessions are crammed with papers. We talk in the halls of the B-factories and other b quark physics facilities being upgraded or built, of the Large Hadron Collider under construction, of research and development on electron-positron linear colliders. We talk of the upgrades of LEP and the Tevatron that are under way. The reports and discussions at this meeting bring us up to date on the research in elementary particle physics of the four thousand or so physicists carrying out or preparing experiments at almost a dozen accelerator laboratories spread across the world; as well as the thousand or so physicists working on non-accelerator elementary particle physics experiments. We continue to learn a great deal in experiment and theory about elementary particle physics.

Yet I think that most of you will agree with me that in the past one and one half decades we have made little fundamental progress in elementary particle physics. When science reporters are out of the room, I think many will agree that our field has almost stagnated. I am not talking about the future which may be exciting, I am talking about the past fifteen years.

In those years many experiments have been carried out, some precise, some exploratory. Important information has been obtained, for example the mass of the top quark has been found. At the same time favorite theories have been extended and elaborated with ever more mathematics. Yet we know nothing more fundamental about particles and mass and energy and forces, than we knew a decade ago. We speak in code words about this near stagnation, saying that the standard model of elementary particle physics works very well, saying that we have not yet broken out of that standard model.

Of course there is no historical or philosophical standard by which we should measure scientific progress. But as an experimenter with limited time, I am 69 years old, I know that I am impatient. Certainly many of you are impatient. In the last few years I have been thinking about possible reasons for the lack of fundamental progress in elementary particle physics. I see research styles and research organizational modes in our field which are detrimental, which have partially caused the near stagnation. This is the subject of my talk: an experimenter's observations on the present state of elementary particle physics. I shall say nothing about the concurrent near stagnation in theory beyond discussing it's effect on experimenters.

Being old and a Nobel Laureate I realize that I must defend myself against the charge that I am indulging in reminiscences about the good old days. My defense has two parts. First I don't believe they were the good old days. Compared to the physicists of my first years in physics, the new generations of physicists are better trained, know more technology, are more alert, are more open to speculation-and they work as hard.

The second part of my defense is that I am an active experimenter, not a philosopher of science, not a statesman of science. I work in the CLEO Collaboration using the CESR electron-positron collider, building some new equipment and once more studying the tau and other particles. I have been working with my young colleagues Dennis Ugolini on a rare decay mode of the eta meson [3], and Xiaofan Zhou on radiative tau decays. I also work on a small non-accelerator experiment searching for isolated fractional electric charge such as free quarks [4]. My colleague, Eric Lee, is giving a talk at this meeting on this search [5].

2 Getting Good Ideas in Experimental Science

Experimenters make progress in science sometimes by getting new good ideas, sometimes by slowly accumulating data in an area, and sometimes by luck.

What is luck in experimental science? The most obvious kind of luck occurs when a conventional technique turns up new physics. For example strange particles were discovered in cosmic rays using cloud chambers, a conventional technique.

However, luck also comes about when the experimenter has gone in a new direction, perhaps a direction that was not obviously fruitful to her or his colleagues. Thus when electron-positron colliders were first built in the 1960's and early 1970's, it was expected that the production of hadrons in electron-positron annihilation would be of modest interest. There were no thoughts of the Ψ/J or of the D mesons.

Even making progress by the slow accumulation of data comes down in the end to good ideas or luck. In the 1960's there was great emphasis on studying the production of many particles in high energy hadron-hadron collisions. I was interested in that subject, and while I never carried out such experiments, I devoted several chapters to the subject in my 1974 book *High Energy Hadron Physics*[6]. A great deal of data was accumulated on large multiplicity collisions, but nothing fundamental was learned. The emphasis on large multiplicity collisions was not a good idea and it was not a lucky idea.

Good ideas, luck, researching in contrary directions, are all connected with the mysterious processes of creativity in experimental science. The subject of creativity in experimental work intrigues me. I have found the multitude of books on scientific creativity to be mostly useless in explaining the process or in enhancing one's experimental creativity. I don't have a reliable set of rules leading to creativity, good ideas and luck in research, but I have some observations, that I believe are connected to creativity, good ideas and luck in experimental research. I will describe these observations because I believe some aspects of the sociology, funding, and style in experimental particle physics prevent the application of these observations. I welcome your comments, criticisms and your observations from your experience in experimental science.

Observation 1 Most good ideas in experimental science come when actively experimenting, that is, when designing, building, troubleshooting an experiment, or when operating it and analyzing the data. Good ideas rarely occur when writing proposals or in group meetings.

Observation 2 Many good ideas start off as bad ideas, ill conceived and ill understood. The experimenter with the bad idea often needs time and helpful colleagues to turn the idea from bad to good. Group thinking and group meetings usually prevent such a transition.

Observation 3 If you want to move in a new experimental direction it is often wise to start designing and building even though you have not been able to develop a coherent experimental plan. Of course this means you have to somehow avoid having your proposal reviewed or coming before a program or scheduling committee. The goal is to get going so that Observation 1 is activated.

3 A Difficult Field

It is difficult to get good ideas and to move experimental research along rapidly in elementary particle physics; we work in a difficult field.

Observation 4 It is prudent to recognize when an experimental area is difficult. If you don't like working in a difficult area, don't! There are no rewards for suffering in science; this is in spite of the film versions of the lives of Louis Pasteur and Marie Curie.

Experimental elementary particle physics is a difficult field for two obvious reasons. First as most of us have explained endlessly in modern physics courses for non-scientists and in public lectures, we cannot directly examine or dissect an elementary particle. We must proceed by studying particle collisions, particle decays, and a few static properties of the particles. The second obvious reason is that most of our experiments require costly, elaborate accelerators and apparatus; and experiments may take a decade to carry out.

There is also not an obvious reason for our field being so difficult. Elementary particle physics is *fundamental but narrow*; this narrowness leaves little space for playing with apparatus, for roaming thru neighboring technical subjects, or for just experimenting. In a little while I'll discuss the difference between experimenting and experiments, particularly when the Experiments come with a capital E. I will illustrate the narrowness of elementary particle physics by contrasting it with fluid mechanics.

In 1994 I became interested in fluid mechanics because our experiment searching for fractional charge requires the rapid and consistent production of small liquid drops, less than 10 micrometers in diameter [3]. Fluid mechanics like elementary particle physics has fundamental problems such as the nature of turbulence and the mechanism of vortex formation. But fluid mechanics has many other aspects: applied research in liquids and gases, connections with statistical mechanics and physical chemistry, enormous numbers of engineering applications, observation of phenomena in everyday life. A researcher may specialize in one area of fluid mechanics, but she or he is free to roam and play and experiment and study and observe in other areas of fluid mechanics, while still working in their chosen area.

Observation 5 The freedom to roam and play and experiment and study in other parts of an experimenter's chosen field nourishes and stimulates the mysterious mental processes which lead to good ideas.

Experimenters in elementary particle physics do not have much freedom within the subject itself. They can roam and play and experiment in detector invention and development, more about this in Section 7. But the physics of the standard model is an almost isolated subject. All that we have learned in this century about elementary particle physics has led us to formulate a few deep questions for the decade ahead. What laws fix the three generations and the properties of the particles in those generations? What are the properties of neutrinos? Are there other particles? Are there other forces beyond the four we know? How are quantum mechanics and gravitation to be unified?

These are wonderful and fundamental questions but they have nothing to do with other physics subjects such as condensed matter physics or plasma physics. The only connected subject for the experimenter is astrophysics.

In contrast those experimenters who have devoted themselves to accelerator physics have much more freedom because accelerator physics is connected to many other basic fields such as plasma physics, and because there are many applications. I think this is the reason there have been so many good ideas in accelerator physics in the past fifteen years. I don't see stagnation in accelerator physics.

An aside on theoretical physicists. Those who work in the theory of elementary particle physics are more fortunate than their colleague experimenters. The workers in theory are able to roam and to play and to study in other areas such as nuclear physics and condensed matter physics. Of course they have no guarantee that the theoretical advance which will take our subject beyond the standard model will have analogies in these other fields. Their excursions may be unproductive.

4 Experiments and Experimenting

I said that experimenters in elementary particle physics do not have much room within the subject itself to roam and to play. I have two prescriptions for using what metaphorical research space is available, perhaps even enlarging that space. These prescriptions are based upon the distinction between Experiments with a capital E and experimenting.

Almost all our Experiments of the last decade, of the present and of the near future consist of a set course of experimental work based upon a prescribed list of goals set very early in the course. You all know the sequence. A collaboration presents a letter of intent to a program committee. This document, or the proposal which follows it, tells about the physics objectives, describes the apparatus to be built, the accelerator time needed. If the apparatus is large this must be followed by conceptual design reports, technical design reports, and many reviews. Eventually the apparatus is built and commissioned, data acquisition begins and results begin to appear. Later in the life of the collaboration there may be a new proposal for an upgrade to give renewed vigor to the enterprise.

Most measurements in elementary particle physics are obtained thru this Major Experiment mode, and indeed must be done this way. This is obvious, I have no argument with the Major Experiment mode in our field. Indeed, if a general purpose detector has been built, there is some freedom in these Experiments for the individual physicist to deviate from the prescribed goals of the proposal. She or he may even turn out to be lucky, but they will have to have a strong ego to work in some eccentric direction while all their colleagues are looking for the Higgs particle.

However there are precious aspects of the practice of experimental science which are absent in the Major Experiment research mode. You cannot set out to measure phenomena or to seek new phenomena which program committees do not like; unless you have managed to conceal your unconventional goals in a conventional proposal. You cannot change your goals partly thru the course of the Experiment's construction or operation when as a result of Observations 1 or 2 you have found a better way to do the experiment. You cannot deviate from the plans prescribed in the proposal when you get a good idea which would take off on a course 90 degrees to the prescribed course. In addition, if all the energy and time of researchers has been committed to the large, long Experiment; they dare not take time to roam or play in other parts of even the narrow field of elementary particle physics. Hence Observation 6.

Observation 6 When we work on a Major Experiment most of the time we are not experimenting.

Observation 7 Try to work in two experimental directions at the same time, one may be a Major Experiment, the other should be a small, low visibility effort in which you can roam and play and easily change course. The pace of this other experimental work must not be set by the need to meet a program or accelerator schedule. This is the way to combat the narrowness of elementary particle physics.

Of course Observation 7 is not for everyone or every time. Many experimenters are happiest and most productive when concentrating on one experiment. And there are some periods in experimental work when one must work full time on a single experiment to get it started or to complete the research.

Observation 8 Do not work on two Major Experiments at the same time, this prevents all experimenting and creativity.

5 10% of the Money and 30% of the Time

At present a minority of experimenters in elementary particle physics can follow Observation 7; it is primarily those who have secure positions and have access to some part of large research or apparatus construction budgets. For most young physicists Observation 7 is a dream. But the elementary particle physics community can change its overall research style to allow young experimenters to follow Observation 7. For the foreseeable future Major Experiments will dominate experimental elementary particle physics. I propose that these Experiments be slowed somewhat by taking 10% of our experimental budgets and letting those who wish use that money for experimenting in elementary particle physics in a free and unrestricted way. I have worked on enough Major Experiments and I have been on enough review committees to know that there will be fierce opposition to reducing the funds for a Major Experiment by 10%. The claim is always made that the last 10%, or even 5%, of the funds are crucial. Such claims are good science politics but they are not good for experimental science.

Experimenting also takes time, more time than money. Therefore those who wish to follow Observation 7, and I am thinking primarily of the young physicists, should be allowed, or indeed expected, to spend 30% of their time in experimenting. Hence Observation 9.

Observation 9 Experimental progress in elementary particle physics will be more rapid, we will be more likely to get good ideas or with luck to break into new physics in elementary particle physics if experimenters, particularly those involved in Major Experiments have funds and time for experimenting. A useful rule is 10% of the funds and 30% of the time to be spent experimenting.

A warning. The money must not given out by anything resembling a review committee or program committee, that committee attempting to judge ideas with the best potential. Remember Observations 1 and 2, "Most good ideas in experimental science come when actively experimenting, that is, when designing, building, troubleshooting an experiment, or when operating it and analyzing the data. Good ideas rarely occur when writing proposals or in group meetings. Many good ideas start off as bad idea, ill conceived and ill understood. The experimenter with the bad idea often needs time and helpful colleagues to turn the idea from bad to good."

6 The Dictatorship of Theory

Observation 10 The ideal relationship between theory and experiment is that theory should be a good traveling companion to experiment in the exploration of nature. Sometimes theory should lead experiment, sometimes theory should follow experiment, but theory should never dictate reality to experiment.

Unfortunately this maxim does not describe the present relationship between theory and experiment in elementary particle physics. The frustrations of the past decade in our field have led to two kinds of misuse of unproven theories in physics. One kind of misuse is illustrated by the dominance of the supersymmetry hypothesis. Supersymmetry is an interesting system and if it proves to be a correct description of elementary particles, it will be a great stride forward. But its correctness has not been demonstrated by experiment. Its correctness is not demonstrated by claims that there is no adequate competing theory or by pointing out that it is a very reasonable hypothesis.

Yet every proposal for a new accelerator and most proposals for Major Experiments cite the supersymmetric hypothesis as a crucial justification. There would be nothing wrong with this if it were just a convenient way to fill the proposal's theory section. Unfortunately the dominance of the supersymmetric hypothesis distorts the design of experiments and inhibits experimenting, particularly experimenting by young researchers.

An example from my own work. When thinking about leptons I sometimes speculate that there may be a force restricted to leptons just as the strong force is restricted to quarks. Perhaps such a lepton-specific force[7] may be related to the drastic difference between the mass of a charged lepton and the mass of its neutrino? Consider two competing experimental proposals with one a search for a lepton-specific force and the other a search for supersymmetric particles. Which will a program committee approve?

The present emotional need for our community to elevate *without experimental proof*, a reasonable hypothesis into an almost proven theory is understandable. We have been working very hard for fifteen years, we want to understand better the nature of mass and force and energy, we deserve to understand better the nature of mass and force and energy. But wishes will not make it so.

Observation 11 We will make progress faster in elementary particle physics if we maintain strong skepticism towards a dominant but unproven hypotheses, thus keeping our minds and particularly young minds open to other speculations, and thus allowing broader justifications for Experiments and experimenting.

I began this section with the statement that there are two kind of misuse of unproven theories in present elementary particle physics. The second kind is exemplified by string theory. String theory at present is an elaborate, and they tell me beautiful, mathematical system which may or may not have anything to do with reality. No one knows how to do an experimental test. You may ask what is wrong with letting the mathematically minded in our field work and play with string theory. There is nothing wrong with the working and the playing.

What is detrimental is the way we have allowed our young physicists to believe that string theory research is the most desirable kind of research, what is wrong is that we take so seriously something which we can't approach experimentally. It is also wrong the way we have allowed, even encouraged, science writers to popularize string theory as the major work in our field.

Observation 12 There is nothing wrong with working on speculative ideas, whether mathematically elaborate and beautiful or not. It is detrimental to experimental progress to turn the heads of young physicists with such claims; it removes them from the reality of what can be tested.

Observation 13 It is also detrimental to the entire community to portray speculation to the public as almost proven theory. There is already enough public confusion about the nature of scientific work.

7 Technological Dreams

I have been discussing what might be done to improve our progress in elementary particle physics within, roughly speaking, the present technology of experimental equipment. I have also noted that at present we see the most inventive and creative developments in accelerator physics and engineering.

We have not done nearly so well in detector technology in the past fifteen years. The only substantial innovation has been the use of solid state particle detectors. In the rest of particle detector technology we have made slow incremental improvements. In the new detectors, those either recently constructed or under construction, the increased power comes from increased size and increased number of channels, not from substantial inventions.

Observation 14 Our failure to devise radically new detector technology bears substantial responsibility for the near stagnation of experimental elementary particle physics.

I'll give you an example from my present tau lepton research. We know all decay modes of the tau which have branching fractions larger than 0.5%, and we have measurements on other decay modes with branching fractions as small as 10^{-4}, some even smaller. But I would like to explore the reverse experimental question. I would like to study 10^8 tau pairs produced in electron-positron annihilation, the events being positively identified as tau pairs by tagging one tau in each pair. I would like to *identify* the decay mode of *every* tau on the untagged side, looking for unexpected decays of the tau. A very open way to look for new physics in tau decays. Of course this is a technological dream, there is no detector existing or planned which has the necessary qualities. We cannot *positively* identify every tau pair. In every detector, existing or planned, 10% or 20% or 30% of tau decays cannot be identified because of poor measurements or undetected decay products or incorrectly identified decay products.

And so I dream of an apparatus with very close to 100% detection efficiency for all charged particles and photons, with 10^{-6} particle misidentification probabilities, with an almost perfect tau pair identification system.

Observation 15 We need creativity and good ideas and luck in apparatus invention and development just as we do in elementary particle physics experimental research itself. The prescription for 10% of the money and 30% of the time would be of great value here.

8 Last Words

In elementary particle physics I am a frustrated optimist, but still an optimist. My technological dreams will come true, there will be much better detectors, there will be new types of accelerators, we will get astonishing new and good ideas, and we will be lucky.

Acknowledgments

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